GENERAL COMMENT

Transport Canada would like to remind the proponent that mooring buoys are considered 'works' therefore the placement of moorings will be subject to review under the *Navigable Waters Protection Act* (NWPA). For further information, the proponent should consult the Transport Canada publication "An Owner's Guide to Private Buoys – TP 14799E" available in http://www.tc.gc.ca/eng/marinesafety/tp-tp14799-menu-634.htm.

UPDATED PROJECT DESCRIPTION

Offshore - 3. Alternate Offshore Loading System
A side-by-side risk profile for the two alternatives should be completed to demonstrate relative risk. This would include a discussion of operational limits, failure pathways, and mitigations.

SPECIFIC COMMENTS

EMCP Comment 4: C-NLOPB 3
"*Cuttings from the surface casing will be returned to the platform and discharged" and "Cuttings cannot be transferred for reinjection because of hydrostatic head*." Once cuttings are on the platform, hydrostatic head should not be an issue as cuttings can be discharged to a pump, which then could move cuttings to a higher elevation. Since the platform is in the early design stages, relocating parts or all of the reinjection system may be possible which would facilitate reducing hydrostatic head. The proponent should provide more reasoning than simply hydrostatic head as to the impediments to re-injecting cuttings generated during drilling with water-based muds.

The proponent's assertion that cuttings in the third section of hole tend to be tacky is based on the proponent's present understanding of the geology. The proponent should be prepared to evaluate the geology to determine if their assertion is factual. The proponent is asked to describe what is meant by "large surface systems" to dissolve the cutting prior to routing to subsurface injection.

While "injection of large volumes of WBM cuttings potentially poses a risk for out-of-zone fracture and the subsequent loss of containment of the NAF materials" the referenced capacity limit has not been demonstrated in any geological study provided by the proponent. The proponent should be prepared to reassess reinjection of WBM cuttings over the life of the project if geology permits.

EMCP Comment 5: C-NLOPB 4
The response lacks technical details to show that natural convection, as described, would occur and the flow sufficient to prevent anaerobic activity in the GBS. Please provide the technical aspects or studies, including calculation, of how the conclusion of natural convection would create a sufficient flow of seawater to eliminate anaerobic action in the GBS. The response should also consider that the cutting pile in the GBS would be a

source of anaerobic bacteria, which could contribute to the presence of anaerobic bacteria in GBS seawater.

EMCP should provide a technical discussion of how sufficient convective flow will be achieved to prevent anaerobic conditions from developing.

EMCP Comment 6: EC 1
The proponent has alluded to a risk assessment completed in regard to gas recovery. Please provide information on the risk assessment completed.

EMCP Comment 7: C-NLOPB 5
The response in part addresses the request in that the proponent looked at additional equipment for produced water treatment to meet the OWTG 2002. The proponent has determined that (comparatively) more advanced treatment will be required at Hebron to meet the performance targets for discharge oil-in-water as set out in the OWTG (2002). However, there is no discussion of achieving better performance than that indicated in the guidelines.

The statement on page 13 (response to comment 8) that at start-up the facility will have "best commercially available and proven treatment technology" should be followed by a commitment to periodically review commercially available treatment technologies to determine if better performance could be achieved.

EMCP Comment 8: C-NLOPB 6
The produced water volume on page 10 (45,000 m^3/d) and on page 3 (56,000 m^3/d) do not agree.

Please provide a copy of ExxonMobil's "initial assessment of produced water re-injection", "injectivity study" and the "initial study of Pool 1 (Ben Nevis reservoir) souring susceptibility", all of which are mentioned in the response.

The proponent states: "*The overall conclusion of the Project's evaluation is that disposal of produced water into a non-producing formation is not feasible nor the preferred option*." The proponent has not shown how it concluded re-injection into a non-producing formation is not feasible or why it is not the preferred option. The proponent should consider in its response if it is possible to dispose of some of the produced water into a non-producing formation thereby reducing the projects overall environmental footprint, and if rejection into a non-producing formation would allow time to fully examine re-injecting produced water into the reservoir (possibly avoiding discharge of produced water). When looking at reinjection of produced water into the reservoir the proponent should also examine the possibility of reinjection into depleted parts of the reservoir or other such parts where produced water would likely have minimal effect on the reservoir.

In examining the additional emissions, the proponent could have compared the emission for injection into a disposal formation to those of re-injecting into the reservoir to show there is a considerable difference in emissions. When looking at additional emissions, the

consequences of the additional emissions should be weighted against the effects produced water may have on the receiving environment.

The Proponent does not mention if sulphite removal or cooling is necessary, and if needed, has it been considered in the topsides design? Also will the well and process equipment be designed to handle produced water so modification will not be required later.

The Proponent also does not seem to have considered the possibility that produced water could be re-injected into separate parts of the reservoir either where water injection has not commenced or where the part is nearing depletion. The proponent should consider discussing the possibility. The potential to dispose of a portion of the produced water in a disposal reservoir (rather than all 366 million m^3) or into depleted portions of the producing reservoir, was not discussed nor was an incremental CO_2eq value assigned as compared to CO_2eq for PWRI for pressure support. The all-or-nothing approach we see here is a familiar one

The impact of warm produced water on injectivity is discussed (page 11) but options available to cool produced water via external (ex. seabed cooling) loops or internal loops (e.g. to dump heat to the GBS cell to aid convection (see comment 5 above)) are not discussed.

Since there is evidence that produced water and seawater reactions occur rapidly after mixing, the proponent should contemplate how a mixed system could be provided with sufficient residence time before injection to allow particulates to evolve for filtration.

EMCP Comment 9: C-NLOPB 7
See Response to ExxonMobil Comment 8.

EMCP Comment 10: C-NLOPB 8
Proven technology implies the proposed design utilizes flare technology currently in use. Since the technology is in use, there should be information available on its performance. This data could be used to predicate emissions from the flare when the low-pressure compressor is down for maintenance. The proponent should also qualify what is meant by minimize smoke production. Does minimize mean there will black smoke and if so what intensity and flow can be expected and how these periods may affect air quality.

EMCP Comment 11: EC 02
The Proponent has indicated that they are still too early in the design stage to provide quantitative detail regarding process system venting and flare design. EC is satisfied with this response at this stage; however, we would like to see these items identified for further review when the detailed design is available.

EMCP Comment 12: DFO 1
Information on the original Hibernia bund wall disposal zone should only be incorporated if this area will actually be used for spoils disposal associated with the Hebron project. In

either case, the following text provided in the response by the proponent should also be included in the CSR:

"*The disposal area in Great Mosquito Cove is unknown at this time. Work is ongoing to identify an area that has the least potential for habitat disturbance and that can accommodate the volume of spoils to be disposed. Based on preliminary review of the bathymetry and fish habitat information for Great Mosquito Cove, a likely candidate area is located at approximately 40 to 45 m water depth on the south side of Great Mosquito Cove. EMCP will consult with DFO regarding the selection of the spoils disposal area. In addition, Transport Canada requirements regarding navigability of water channels will be included in the selection process.*"

EMCP Comment 13: DFO 2
"*Dredging for tow-out from the deepwater site to the offshore location*" should be included as a project activity in Tables 7-9 and 7-11.

EMCP Comment 15: EC 03
The proponent has indicated that the precise location of an ocean disposal site (or sites) is not known at present and this is reasonable given the nature of the project. The CSR has satisfactory descriptions of the general areas in which these sites might be located and has provided discussions of the expected environmental effects and mitigations.

If an application is made for a disposal at sea permit under these circumstances, the information in the CSR, together with details provided in the permit application, may prove sufficient to satisfy the requirements of an assessment which is triggered by the permit application. The proponent's response to Comment 15 is satisfactory.

EMCP Comment 18: C-NLOPB 9
The reference to Section x.x should be complete.

EMCP Comment 24: EC 10
See EC Comments on Appendix A for Response.

EMCP Comment 28: EC 14
EC-14-a and b. The inclusion of additional information and revised/updated wave statistics as requested is appreciated. ICOADS was used as the source of platform wind data. **REQUEST:** Please explain why the presumably more complete archives of the operator or C-NLOPB (please describe) are not used for the analysis of platform winds, for the more long-term platforms. It should be noted that use of the ICOADS trimming flags, both the standard (as indicated in the Oceans Ltd report) and even the enhanced, will exclude a significant percentage of valid extreme wind observations measured by platforms. This flagged data should be considered in future analyses. Note that in the two tables in this response for wave data from Hibernia, it may not be appropriate to combine the 1988-1989 wave radar data with the earlier wave buoy data as the two different observation systems give somewhat different wave distributions. This should be considered for future analyses.

28-EC-14-c. **REQUEST:** Please include a full reference for Berek and Wang 2009, given the important result that increases the extreme wave criteria. Also please elaborate on the source of the wave data that was used to calibrate the MSC50 significant wave heights.

28-EC-14-d. The response asked about White Rose data post August 2007 – however the original comment did not intend to imply data existed beyond that date but that within the 2003-2007 period there were additional observations. These seem to have been accessed in the revised tables given in this response. The response also says that other Hibernia platform data do not appear to be online. While the MIROS wave radar data do not seem to be archived at ISDM, the wave radar is apparently still in use. It was presumably the source of the wave data included in the ICOADS archive for the Hibernia GBS, with results given in the response to section f. **REQUEST**: Please clarify the source of the later wave data in the MANMAR reports from Hibernia archived in ICOADS, and explain the reason that operator or C-NLOPB archives are not accessed for the platform wave data, when it is not available in the ISDM archives.

28-EC-14-f. The response indicated that data from buoy 44153 when it was deployed near Hibernia, winter 1997/1998, was not used because much of the wave data are flagged as erroneous. This flagging was because of a wave processing error in the onboard wave processor which affected the band-averaged spectral data from which ISDM calculates significant wave height (archived as VCAR), but the error did not affect the buoy-reported significant wave height, archived in ISDM as VWH$. The VWH$ values are useable and may be helpful for any further analysis undertaken for the area.

EMCP Comment 32: C-NLOPB 13
The treatment of "statistical information and probabilistic modeling" of loading scenarios may be reviewed during the technical review associated with the development plan or authorization.

EMCP Comment 33: C-NLOPB 14
The inconsistency noted was in the numerical difference in average occurrence rates provided in each section.

EMCP Comment 34: C-NLOPB 15
No bibliographic entry for the "Rudkin *et al* 2005" reference in the response appears in the original nor in the response document and should be provided.

The discussion still is very general and it is not obvious that any concerted effort was made to address the comment.

EMCP Comment 43: C-NLOPB 20
The request is related to the emissions from equipment and not if emissions will meet ambient air quality standards. The question posed is how the proponent will reduce, minimize, emission from equipment, what mitigations and technologies are available to so that emissions from equipment are as low as possible.

EMCP Comment 44: HC 3
Given that the specific VOCs to be released are not known, it is unclear how the risk to human health from exposure to those substances will be evaluated. Please provide a discussion about the following:
• Will VOCs be monitored as individual substances or as total VOCs?
• If VOCs will be monitored as individual substances, how will the individual substances be determined?
• How will VOCs be measured and reported (i.e. as releases in tonnes/year or as concentrations in mg/m3)?
• How will the health risk to humans (including workers and any other people in the area such as fisherpeople) be evaluated (e.g. comparison of emission concentrations to published toxicological reference values (TRVs) for human exposure/occupational exposure limits)?

EMCP Comment 54: C-NLOPB 23
The proponent has not addressed the request; it has not stated why it is necessary to flare excess gas during well testing or what the technical impediments are to processing the gas and re-injecting it.

Where capacity exists in the production train to capture reservoir gas, that capacity should be available for all normal course of business operations (e.g.. cleaning up wells). This would apply to the test separator and associated equipment that is permanently installed on the installation. Flaring reservoir gas during well cleanup where the facilities exist to capture and re-inject is both an environmental and resource conservation issue.

Unless there is a risk-justification for flaring from well clean-up and testing, that gas should be captured and reinjected.

EMCP Comment 56: C-NLOPB 24
The response is acceptable however, the completeness of the table will be reviewed when updated.

EMCP Comment 61: EC 30
EC is looking for information related to emissions from upset scenarios of a more catastrophic nature (i.e. major blow out that might burn for some days).

EMCP Comment 66: DFO 6
The proponent has neglected to address the third bullet. The revised text still claims that, "*The distribution of small crabs is not well documented…*", however, as indicated in our original comment, distribution of small snow crab (juveniles/adolescents) is documented in the CSAS Research Documents, which are produced in accordance with annual assessments of the species. The reference for the most recent CSAS Research Document is provided below. The CSR should be revised to include information from this document.

E. Dawe, Mullowney, D., Stansbury, D., Hynick, E., Veitch, P., Drew, J., Coffey, W., Colbourne, E., O'Keefe, P., Fiander, D., Skanes, K., Stead, R., Maddock-Parsons, D., Higdon, P., Paddle, T., Noseworthy, B., and Kelland, S. 2010. An Assessment of Newfoundland and Labrador Snow Crab (*Chionoecetes opilio*) in 2008. DFO Can. Sci. Advis. Sec. Res. Doc. 2010/016. iv + 183p.

EMCP Comment 69: DFO 7
It should be noted that the scientific name for Snow Crab is incorrect; the correct spelling is *Chionoecetes opilio*. This should be corrected in the next draft of the CSR.

EMCP Comment 72: DFO 10
The proponent has neglected to fully address part c) of this comment. The text still indicates that, "*Recently-settled juveniles (<30 mm, carapace width) prefer a mud substrate…*". As indicated in our original comment, most recently-settled juveniles appear to settle on shallow hard substrates. The text should be revised to reflect this information.

EMCP Comment 75: DFO 13
This response is considered inadequate. This comment was intended to elicit a thorough description of the potential environmental effects within the text, based on the information provided in the tables of Section 7 as there are more environmental effects listed in the tables for various project activities than are discussed within the text. For example, although Table 7-11 indicates that a change in habitat use could result from the following 14 nearshore activities: bund wall construction, in-water blasting, drydock dewatering, concrete production, vessel traffic, lighting, re-establishment of moorings at the deep water site, dredging, bund wall removal/disposal, tow-out to deepwater site, completion of GBS construction and mating of topsides, hook-up and commissioning of topsides, surveys and tow-out from deepwater site, the text in Section 7.5.1.3: Change in Habitat Use (Nearshore) only details the following six nearshore activities as causing a change in habitat use: bund wall construction, in-water blasting, vessel traffic, lights, dredging and surveys. It should be noted that text does not need to be duplicated between sections; however, a cross reference should be provided.

EMCP Comment 77: DFO 17
The response does not indicate whether the CSR will be revised to reflect the information provided. The text provided by the proponent in the response should be incorporated into the CSR.

EMCP Comment 78: DFO 14
This response is considered inadequate. While DFO acknowledges the proponent's commitment to the development of a Fish Habitat Compensation Strategy, it is necessary to include, at the very least, high level details on the potential compensation options to be implemented within the CSR. This is required as fish habitat compensation is considered a mitigation under CEAA (the single most important mitigation from the perspective of the Habitat Provisions of the *Fisheries Act*), which must be applied to ensure that any residual adverse environmental effects are not significant. Until such a time as that

information is included in the CSR, DFO will be unable to make a determination of the significance of effects, with respect to the potential impacts on fish and fish habitat, within the context of CEAA. Additionally, DFO has provided further direction for the breadth and depth of information on a fish habitat compensation strategy, to be included in the CSR, to ExxonMobil in separate correspondence.

EMCP Comment 82: EC 37
The response should be clarified as follows: "SBM cutting reinjection is not technically feasible for MODU drilling and SBM cuttings will be discharged overboard after treatment in accordance with the OWTG. ".

EMCP Comment 83: C-NLOPB 26
It may be better if, at the noted location and in addition to the OWTG reference, the reader was directed back to discussion in the appropriate sections for each discharge.

EMCP Comment 84: C-NLOPB 27
The response relies heavily upon the CAPP (2001) reference. CAPP (2001) originally was prepared in 1999-2000 as a submission to the review of the 1996 Offshore Waste Treatment Guidelines. Given that cuttings dryers were in limited use at that time, it is not entirely clear that the statements in CAPP 2001 corresponded to lower synthetic-on-cuttings values.

The response should be re-examined.

EMCP Comment 90: DFO 22
The response is considered adequate, provided that it is included within the CSR.

EMCP Comment 94: C-NLOPB 29
The response in the "Response to Review Comments" document is acceptable, however, it is not clear whether it will be inserted into the CSR, and if so, where.

EMCP Comment 103: C-NLOPB 30
This response is not acceptable. The reference to a spill being "unlikely" presumably is intended to refer to large-volume spills and this distinction should be made clear. Experience indicates that small-volume spills cannot reasonably be considered unlikely. A word like unlikely must be qualified or bracketed (i.e. $<10^{-3}$) and a term like spill must be bracketed (i.e. >10 litres). Spills will be reported on the C-NLOPB website and experience indicates that "small volume" spills are not unlikely. Words like "unlikely" will be seen in a much harsher light when reviewed in an operational context.

The CSR states, on page 14-13 "The C-NLOPB also provides a statistical record of spills of greater than 1 L but less than 1 barrel (159 L), and of spills of 1 L and less. These are presented in Table 14-13, but are not used for any predictive purpose." This issue is obviously not well understood by the proponent when statements like "an oil spill, although unlikely, could potentially occur" appear in the response to a question about the use of the term "unlikely" and data on spills <159 litres is deemed insignificant.

The proponent should revisit 14-13 and make appropriate reference to data collected locally. From the CSR reported spill data 1997 to 2009:
Spill size > 0 and < 1 litre = 213,
Spill size ≥ 1 and <159 litres = 138
Spill size ≥ 159 but <999 litres = 19
Spills > 999 litres = 1
Total = 371

This number differs from the C-NLOPB published statistics. As well, the proponent seems to have excluded spills of synthetic based mud from some of the data. This is not appropriate as these are reported as spills of hydrocarbon and operators have been prosecuted for the discharge of synthetic based mud.

EMCP Comment 106: C-NLOPB 31
This response is not acceptable concerning the discussion of spectral mitigation. The mitigations referred to (response to Comment 102) are not related to spectral mitigation. If EMCP does not want to consider spectral mitigation, they should answer "No" to the question asked.

The proponent has discussed the non-applicability of the cited studies but has not proposed any alternative information. Was there absolutely nothing of relevance in the studies? Are there any alternative studies that might contain useful and relevant information? Has EMCP conducted a thorough literature review to determine that no research is available?

If you have dismissed Poot *et al*. 2008, as a reference, why are you adding it in response to comment 107?

EMCP Comment 114: GF 19
"*Each prediction did not have a corresponding level of probability of occurrence and scientific certainty*."

The last offshore oil production project in 2000, the White Rose, did provide both of those evaluative criteria (probability of occurrence and scientific certainty) to EACH PREDICTION in the EA, not each phase. So I see this change as one which provides less detail rather than more.

EMCP Comment 119: EC 47
A review of the avian toxicology literature cited below demonstrates that the CSR statement "**It appears that direct, long-term sublethal toxic effects on marine birds are unlikely**" (made in section 9.5.4.3) is not scientifically accurate or defensible. The citations provided in that paragraph of the CSR are out-of-date. The entire paragraph should be re-written to reflect the literature below, or removed entirely.

Alonso-Alvarez, C., Munilla, I., López -Alonso, M., Velando, A., 2007. Sublethal toxicity of the Prestige oil spill on yellow-legged gulls. Environment International 33,

773-781.

Balseiro, A., Espí, A., Márquez, I., Pérez, V., Ferreras, M.C., García Marín, J.F., Prieto, J.M., 2005. Pathological features in marine birds affected by the Prestige's oil spill in the north of Spain. Journal of Wildlife Diseases 41, 371-378.

Bernanke, J., Köhler, H.-R. 2009. The impact of environmental chemicals on wildlife vertebrates. Reviews of Environmental Contamination and Toxicology 198: 1-47.

Briggs, K.T., Gershwin, M.E., Anderson, D.W. 1997. Consequences of petrochemical ingestion and stress on the immune system of seabirds.
ICES Journal of Marine Science 54 (4) : 718-725.

Briggs, K.T., Yoshida, S.H., Gershwin, M.E. 1996. The influence of petrochemicals and stress on the immune system of seabirds. Regulatory Toxicology and Pharmacology 23 (2): 145-155.

Carls, M.G., Heintz, R., Moles, A., Rice, S.D., Short, J.W. 2005. Long-term biological damage: What is known, and how should that influence decisions on response, assessment, and restoration? 2005 International Oil Spill Conference, 4389-4393.

Esler, D., Trust, K.A., Ballachey, B.E., Iverson, S.A., Lewis, T.L., Rizzolo, D.J., Mulcahy, D.M., Miles, A.K., Woodin, B.B.R., Stegeman, C.J.J., Henderson, J.D., Wilson, B.W., 2010. Cytochrome p4501a biomarker indication of oil exposure in harlequin ducks up to 20 years after the Exxon valdez oil spill. Environmental Toxicology and Chemistry 29, 1138-1145.

Gentes, M.L., McNabb, A., Waldner, C., Smits, J.E.G., 2007. Increased thyroid hormone levels in tree swallows (*Tachycineta bicolor*) on reclaimed wetlands of the Athabasca oil sands. Archives of Environmental Contamination and Toxicology 53, 287-292.

Giese, M., Goldsworthy, S.D., Gales, R., Brothers, N., Hamill, J., 2000. Effects of the Iron baron oil spill on little penguins (*Eudyptula minor*). III. Breeding success of rehabilitated oiled birds. Wildlife Research 27, 583-591.

Holmes, W.N., Cavanaugh, K.P. 1990. Some evidence for an effect of ingested petroleum on the fertility of the mallard drake (*Anas platyrhynchos*). Arch. Environ. Contam. Toxicol. 19 (6): 898-901.

Iverson, S.A., Esler, D., 2010. Harlequin Duck population injury and recovery dynamics following the 1989 Exxon Valdez oil spill. Ecological Applications 20, 1993-2006.

Jane Harms, N., Fairhurst, G.D., Bortolotti, G.R., Smits, J., 2010. Variation in immune function, body condition, and feather corticosterone in nestling Tree Swallows (*Tachycineta bicolor*) on reclaimed wetlands in the Athabasca oil sands, Alberta, Canada. Environmental Pollution 158 , 841-848.

Jessup, D.A., Leighton, F.A., 1996. Oil pollution and petroleum toxicity to wildlife. In: Fairbrother, A., Locke, L.N., Hoff, G.L. (eds.) Noninfectious diseases of wildlife, 2nd ed., Iowa State University Press, Ames, Iowa. pp. 141-156.

Kamata, R., Takahashi, S., Shimizu, A., Morita, M., Shiraishi, F. 2006. In ovo exposure quail assay for risk assessment of endocrine disrupting chemicals. Archives of Toxicology 80, 857-867.

Leighton, F.A. 1993. The toxicity of petroleum oils to birds. Environmental Review 1 (2): 92-103.

Leighton, F.A. 1986. Clinical, gross, and histological findings in herring gulls and Atlantic puffins that ingested Prudhoe Bay crude oil. Vet. Pathol. 23 (3): 254-263.

Mearns, A.J., Reish, D.J., Oshida, P.S., Buchman, M., Ginn, T., Donnelly, R., 2009. Effects of pollution on marine organisms. Water Environment Research 81, 2070-2125.

Munilla, I., Velando, A., 2010. Oiling of live gulls as a tool to monitor acute oil spill effects on seabirds. Ibis 152, 405-409.

Newman, S.H., Anderson, D.W., Ziccardi, M.H., Trupkiewicz, J.G., Tseng, F.S., Christopher, M.M., Zinkl, J.G. 2000. An experimental soft-release of oil-spill rehabilitated American coots (*Fulica americana*): II. Effects on health and blood parameters. Environ. Pollut. 107 (3): 295-304.

Oropesa, A.L., Pérez -López, M., Hernández, D., García, J.P., Fidalgo, L.E., López - Beceiro, A., Soler, F. 2007. Acetylcholinesterase activity in seabirds affected by the Prestige oil spill on the Galician coast (NW Spain). Science of the Total Environment 372, 532-538.

Pérez, C., Munilla, I., López -Alonso, M., Velando, A., 2010. Sublethal effects on seabirds after the Prestige oil-spill are mirrored in sexual signals. Biology Letters 6, 33-35.

Rattner, B.A., 2009. History of wildlife toxicology. Ecotoxicology 18, 773-783.

Rattner, B.A., Eroschenko, V.P., Fox, G.A. 1984. Avian endocrine responses to environmental pollutants. J. Exper. Zool. 232 (3): 683-689.

Smits, J.E., Wayland, M.E., Miller, M.J., Liber, K., Trudeau, S., 2000. Reproductive, immune, and physiological end points in tree swallows on reclaimed oil sands mine sites. Environmental Toxicology and Chemistry 19, 2951-2960.

Smits, J.E., Williams, T.D., 1999. Validation of immunotoxicology techniques in passerine chicks exposed to oil sands tailings water. Ecotoxicology and Environmental

Safety 44, 105-112.

Troisi, G., Borjesson, L., Bexton, S., Robinson, I., 2007. Biomarkers of polycyclic aromatic hydrocarbon (PAH)-associated hemolytic anemia in oiled wildlife. Environmental Research 105, 324-329.

Trust, A., Esler, D., Woodin, R., Stegeman, J., 2000. Cytochrome P450 1A induction in sea ducks inhabiting nearshore areas of Prince William Sound, Alaska. Marine Pollution Bulletin 40, 397-403.

Velando, A., Álvarez, D., Mouriño, J., Arcos, F., Barros, Á., 2005. Population trends and reproductive success of the European shag *Phalacrocorax aristotelis* on the Iberian Peninsula following the Prestige oil spill. Journal of Ornithology 146, 116-120.

Wolfaardt, A.C., Williams, A.J., Underhill, L.G., Crawford, R.J.M., Whittington, P.A., 2009. Review of the rescue, rehabilitation and restoration of oiled seabirds in South Africa, especially African penguins *Spheniscus demersus* and Cape gannets *Morus capensis*, 1983-2005. African Journal of Marine Science 31, 31-54.

Wolfaardt, A.C., Underhill, L.G., Nel, D.C., Williams, A.J., Visagie, J., 2008. Breeding success of African penguins *Spheniscus demersus* at Dassen Island, especially after oiling following the Apollo Sea spill. African Journal of Marine Science 30, 565-580.

Zuberogoitia, I., Martínez, J.A., Iraeta, A., Azkona, A., Zabala, J., Jiménez, B., Merino, R., Gómez, G. 2006. Short-term effects of the prestige oil spill on the peregrine falcon (*Falco peregrinus*). Marine Pollution Bulletin 52, 1176-1181.

EMCP Comment 129, EC 46

EC is concerned about the response to several comments which outline the need to include marine bird monitoring in the EEM. Although the response makes it clear that EEM planning is only in the beginning stages, the proposed text change to **"Based on the environmental effects assessment for marine birds, a marine bird EEM component is not contemplated at this stage"** is the core of our general concerns. EC recommends that marine bird monitoring for the following reasons:

In the report, attraction to illumination on structures and vessels during all phases of the Project are predicted to be "…*low in magnitude, geographic extent, duration, frequency when mitigation measures are practiced*". Also, The effects of "…*accidents, malfunctions and unplanned events*…" are predicted to be "…*significant…*", but "…*reversible at the population level*". An EEM that includes marine birds will determine the accuracy of these predictions. Specifically, more data on the distribution and abundance of marine birds in the vicinity of the study area are essential for assessing the accuracy of these predictions. Globally significant concentrations of marine birds are known to use the Grand Banks, and may concentrate in the vicinity of the proposed development site. Although there will be an emphasis on accident prevention, data on

marine birds are required from the development site in order to assess risk and mortality should an accident occur.

EMCP Comment 133: DFO 27
As a point of clarification, Schedules 2 and 3 of SARA contain species which had been assessed by COSEWIC prior to their adoption of new criteria in 1999. When SARA was proclaimed in 2003, the species in Schedule 2 and 3 were to be re-assessed by COSEWIC using the new criteria. Harbour Porpoise has since been re-assessed by COSEWIC in 2006 using the new criteria (as special concern) and is in the SARA listing process. Sowerby's Beaked Whale has also been re-assessed by COSEWIC in 2006 using the new criteria (as special concern) and is in the SARA listing process.

For further information on Schedules 2 and 3 of SARA, please refer to the following link to the SARA Public Registry: http://www.sararegistry.gc.ca/species/default_e.cfm (click "View Schedule 2" or "View Schedule 3" and see the explanatory note at the top of these pages).

EMCP Comment 138: DFO 32
The adequacy of this response cannot be assessed until additional oil spill trajectory modelling is received by the department. However, it should be noted that demersal juvenile Atlantic Cod are also likely to be present in eelgrass and should be included in the assessment.

EMCP Comment 139: DFO 33
The adequacy of this response cannot be assessed until additional oil spill trajectory modelling is received by the department. However, it should be noted that the last part of the sentence should read, "*…which could result in a potential widespread die-off of meadows as well as individual plants.*"

EMCP Comment 193: Alder 4
It is interesting that EMPC has chosen an answer that refers to fundamental statistical theory. Indeed the central limit theorem implies that for any phenomema, where there are many repeated independent randon trials, the mean of the data (but not necessarily the data itself) will tend to become normally distributed [i.e. the distribution of the means of various samples pulled from the data set is a normal distribution] as the number of trials increases. The observed data is distributed in accordance with some unknown distribution (although we may fit distributions of varying utility to it).

A first concern regarding the answer given is that, for any one variable [e.g. frequency of ice occurrence in a grid square], the process may or may not be random or independent for a given year. Secondly, the normalcy of the mean is not necessarily an indication of its appropriateness as a measurement of central tendancy of a distribution. A comparison of the mean and median is an excellent indicator of the centrality of both parameters. The mean is more sensitive to outliers than the median and their comparison is also a good indicator of the symmetry or skew of the data.

Understanding variability is key to understanding the meaning of data. Rather than focusing on the central tendancy, it may be more useful to think about the likelihood of a proportion of data falling inside or outside some boundary which determines the likelihood of an event being manageable or not.

EMCP Comment 199: NLDEC 2
A) NLDEC accepts response.

B) The linear interpolation scheme used to estimate hours between three-hour surface observations is acceptable. However, the question of whether or not the meteorological parameters needed run AERMET are available from Hibernia was not addressed. It is quite likely that ceiling height and cloud cover data in particular, are not available from Hibernia but were obtained from St. John's airport. If so, then an explanation is required as to the appropriateness of merging Hibernia data with St. John's data and the effects this will have on the model outcomes.

C) The response does not answer the question. In the report, the location of the Hibernia platform point source (669419 E, 5179807 N (page 14)) is identical to the Hibernia receptor location (pages 17 thru 27). For Terra Nova, the point source and the receptor are 1metre apart. In both instances, this implies the receptor is inside the stack, which clearly is illogical and will hence cause modelling anomalies. Please clarify if this is the case and how the results may be impacted.

D) While it is appreciated that there is limited ozone data for the project area to determine a conversion rate from NO to NO_2, assuming that a conversion rate of 15% is adequately protective of the environment is very subjective and likely an underestimation. Using the Plume Volume Molar Ratio Method (PVMRM) as the most advanced algorithm for NO to NO_2 conversion in AERMOD, it can be readily shown, that based on the emission characteristics that were modelled, if, for example, a background ozone level of 20 ppb is assumed under D stability, the conversion rate is approximately 22% (excluding original 10% NO_2 emission rate) about 1 kilometre from the source. Similarly, for example, if a background ozone level of 10 ppb is assumed under D stability, the conversion rate is approximately 32% approximately 2 kilometres from the source. Therefore while a 15% static conversion rate may be somewhat realistic in close proximity to the source, it may be inaccurate beyond 1 kilometre and hence not representative worst-case conditions. The proponent needs to re-evaluate the assumptions made and provide clarity to the potential impacts.

E) NDOES accepts Hebron's response regarding "particular" vs "particulate". With regard to the operation of BPIP, NDOEC does not question the proponent's ability to use the model. The question is whether the proponent is aware that how the data is entered into the model will dramatically affect the outcomes from, the model. For example, a simple 1-tiered 20m x 20m building with 1 stack entered as one building will give a different result that the same building entered as 2 adjacent 10m x 20m buildings. This compounding error within BPIP becomes magnified in situations where there are numerous adjacent structures and tiers. If the BPIP inputs were not entered correctly unrealistic results may occur. Can the proponent provide assurances that the downwash effects are accurately modelled and representative of the configuration of the Hebron platform?

F) The model outcomes indicate that 24-hour TSP levels could reach 99.4 $\mu g/m^3$ during peak platform operation. Based on the proponent's response, it is implied that because $PM_{2.5}$ is a

fraction of TSP, and TSP is compliant, therefore $PM_{2.5}$ is compliant. This is illogical. If, for example, all the TSP is $PM_{2.5}$, then regardless of which standard is used for comparison, the standard would be exceeded by a factor of 3 - 4. The proponent needs to provide assurance that the $PM_{2.5}$ standards will not be exceeded.

SECTION 5 OF EMCP RESPONSE

Additional Deficiencies and Editorial Comments from Fisheries and Oceans Canada

A2) 4.3.3 Step 3 – Definition of Significance: Page 4-10 Fish and Fish Habitat
DFO acknowledges that the definition of "*significant effect*" must address both fish and fish habitat; however, this comment was intended to address the fish habitat portion of the definition which is detailed in the following text, "*For potential environmental effects on marine fish habitat, a significant adverse residual effect would be one that results in an unmitigated or non-compensated net loss of fish habitat as required in a Fisheries Act harmful alteration, disruption or destruction (HADD) authorization*." This text should be revised as follows, "*For potential environmental effects on marine fish habitat, a significant adverse residual effect would be one that results in a harmful alteration, disruption or destruction of fish habitat that is so large and/or the fish and fish habitat is of such importance that it cannot be adequately compensated*."

B3) 7.3.1.5 Fish and Shellfish: Page 7-10 (Greenland Halibut)
The revised statement still fails to address the fact that Greenland Halibut can be found at depths greater than 1,500 m and while it is predominantly considered a deepwater species, it can be found at all depths. It is suggested that the statement be reworded as follows, "*Greenland Halibut can be found at depths ranging from less than 100 m to deeper than 1,500 m. While most are caught near the sea bottom at depths of between 200 to 600 m, they can be found at all depths."*

B4) 7.3.2 Offshore: Page 7-16
This response indicates that DFO RV data has been requested by the proponent for 3N, but was not received at the time the response was written. After consulting the DFO Request for Data Transfer records, it was noted that this information was provided to the proponent's consultant (LGL Limited) in March 2010 and September 2010. Please ensure that this information is incorporated into the next draft of the CSR.

B16) Page 7-42
The revision of this statement is acceptable; however, as per DFO's previous comment, the word "*destruction*" should be removed. It should be noted that a change in habitat quality would not constitute a destruction of fish habitat as by definition a "*destruction*" refers to any permanent change of fish habitat which completely eliminates its capacity to support one or more life processes of fish. Therefore a change in habitat quality would more likely result in a "*harmful alteration*", which is any change to fish habitat that indefinitely reduces its capacity to support one or more life processes of fish or a "*disruption*", which is any change to fish habitat occurring for a limited period which reduces its capacity to support one or more life processes of fish.

B23) Page 7-62
While the proponent has acknowledged DFO's comment, there is no indication within the response that the text within the CSR will include the intent to consult with Federal Authorities, including DFO to ensure that the best location is chosen for the disposal area

in order to minimize any adverse effects on fish and fish habitat. This text should be added to the CSR.

B24) Table 7-12

Although the changes to Table 7-9 are acceptable, Table 7-12 and the corresponding text in Section 7 should also be revised as per DFO's previous comment.

F5) 12.5.1.1. Near Shore (Accidents, Malfunctions and Unplanned Events): Page 12-16 (Eelgrass Beds)

With respect to part a) of this comment, it should be noted that any compilations or summaries of studies completed by others are not considered source material. As such, neither Fingas (2001) nor Wright (2002) constitute source material. Rather than cite the information contained in these 'compilation documents', the original study upon which a particular statement is based should be consulted and referenced appropriately. This is necessary to ensure that the conclusions of the original study are accurately represented, free from the potential biases of subsequent interpretations of the work. The correct sources for the information in question should be located, and the text updated as necessary.

With regards to part c) of this comment, the reference provided by DFO in our original comment, Warren et al. in press JEMBE, is now published. The complete reference is provided below for your consideration.

Warren, M.A., Gregory, R.S., Laurel, B.J. and Snelgrove, P.V.R. 2010. Increasing density of juvenile Atlantic (*Gadus morhua*) and Greenland cod (*G. ogac*) in association with spatial expansion and recovery of eelgrass (*Zostera marina*) in a coastal nursery habitat. J. Exp. Mar. Biol. Ecol. 394: 154–160.

SECTION 8 OF EMCP RESPONSE

Appendix A – CIS, EC Comments on Sea Ice and Iceberg Sections

A. Responses to Comments on SECTION 3 of CSR

Response to Comment A-1: unsatisfactory.
Although the requested change from "cyclical" to "variable" was made, a later comment (Comment A-3) asked that the error in the indicated data span "1983-2008" be corrected everywhere to "1971-2000" so that it is consistent with the cited data source, and this was not corrected here in the response to comment A-1.

Response to Comment A-3: unsatisfactory.
While the text changes made to the indicated paragraph are satisfactory, the portion of the comment requesting that that the error in the indicated data span "1983-2008" be corrected everywhere to "1971-2000" so that it is consistent with the cited data source has not been addressed.

Response to Comment A-6: partly satisfactory.
The graph 3-13 now contains even more errors than before. *The ice chart data were not re-checked and the graph data were simply randomly shifted (in the wrong direction).* The correct data are indicated in the table below.

The graph 3-14 is correct. There are a couple of minor differences in interpretation with respect to the table of values given below, but these are acceptable given the ambiguity of the chart data in some cases.

	Frequency of presence of sea ice (max % for category)		Derived ice thickness (max cm for category)	
date	**bottom**	**mouth**	**bottom**	**mouth**
Jan 01	0	0	0	0
Jan 08	0	15	0	10
Jan 15	0	15	0	15
Jan 22	15	15	10	15
Jan 29	15	33	10-15	15
Feb 05	33	50	10	15
Feb 12	33	50	10	15
Feb 19	33	50	10	15
Feb 26	50	66	10	30
Mar 05	50	66	15	30
Mar 12	50	50	15	120
Mar 19	33	50	120	120
Mar 26	15	33	120	120
Apr 02	33	33	120	120
Apr 09	15	33	120	120
Apr 16	15	33	120	120
Apr 23	15	33	120	120
Apr 30	15	33	120	120
May 07	15	15	120	120
May 14	15	15	120	120
May 21	0	15	0	120
May 28	0	15	0	120
Jun 04	0	0	0	0

Response to Comment A-8: unsatisfactory.
Figure 3-14 has been modified to reflect the upper limit of the 70-120cm thickness range. However, the sentence in the text "The bay experiences first-year ice from mid-March through early May, which can range in thickness from 70 to 120 cm." has not been corrected to explain that the 70-120cm range represents *medium* first year ice, but that there is no way to determine from the CIS ice charts that medium first year ice prevails over the thin (30-70cm) or thick (>120cm) first year ice categories.

The sentence "The bay experiences first-year ice from mid-March through early May, which can range in thickness from 70 to 120 cm." should be modified to something like: "The bay experiences first-year ice from mid-March through early May, **which can range in thickness from 30 cm to greater than 120 cm**."

**Additionally, based on the graphs, the sentence "As with the offshore area, most sea ice that occurs within the bay is formed off southern Labrador and drifts south to enter the bay around the mid-March timeframe." is incorrect. While the thickest ice occurs from mid-March to mid-May, the greatest frequency of sea ice begins near the end of February and lasts until mid-March, indicating that the ice enters the bay towards the end of February.

Additionally, the sentence "This analysis includes the sea ice at the mouth and bottom of the bay over the same 25-year period." has not been corrected according to comment A-3. The erroneous 1983-2008 period may represent 25 years, but the Atlas data is actually for 1971-2000 or **30 years.

Response to Comment A-17: mostly satisfactory.
The table is much improved but there is still an error in the 4th row of the table. White ice is thin first year ice, not "young" ice as indicated in the first column of that row. Also, the table now omits any mention of medium and thick first year ice. The following changes (indicated in blue) are suggested:

Table 3-50 Characterization of Sea Ice by Type, Thickness and Age

Ice Type / Stage of Development	**Thickness (cm)**	**Age / Period of formation**
New Ice	<10	**Seasonal ice: Earliest stage of development**
Young (Grey) Ice	10 to 15	**Seasonal ice: generally early season**
Young (Grey-White) Ice	15 to 30	**Seasonal ice: generally early to mid-season**
Thin First-year (White) Ice	**30 to 70**	**Seasonal ice: generally mid- to late-season**
Medium First-year Ice	**70-120**	**Seasonal ice: generally late-season**
Thick First-year Ice	**>120**	**Seasonal ice: generally late-season**

Second-year / Multi-year / Old Ice	**>120**	**Perennial ice**
Source: Meteorological Service of Canada Canadian Ice Service MANICE (2005)		

Response to Comment A-22: partly satisfactory.
While the figure caption has been corrected to say "within 28 km", the text on page 3-65 (second to last paragraph) needs to be corrected as well, as indicated in the comment.

Response to Comment A-23: unsatisfactory.
While the noted error regarding the computed means of the data values was satisfactorily addressed, **several more errors were noticed**.
1) The Source of the data listed under the table is: CIS Ice Charts and Field Observations 2000-2008 and yet the table contains data for 1972-2008.
2) Also, going back and spot-checking the dates/concentrations in the table against the CIS online chart data indicates that the values in this table do not correspond very well or at all with the archived CIS chart data and must come either from somewhere else or the person going through the charts misinterpreted the dates and concentrations in many places. EC does not have time to re-do this entire table for the consulting company. Please re-check the source, the chart dates, the derived data, etc.

Response to Comment A-24: partly satisfactory.
Again, as noted above for A-22, besides the changes already made, the text in this paragraph needs to be corrected from "15 km" to "28 km".

Response to Comment A-27: partly satisfactory.
In the response, it is not indicated whether the issue of the inconsistent reference for Figure 3-36 was addressed. It is not clear if the figure was replaced or just supplemented with additional figures. If the figure and its related text were retained, then the comment "In the text it says the study was conducted over the period 1984-87 by Seaconsult Ltd. (1988), but in the figure itself it says "Data: February through April 1985" and references "Fissel et al. (1985)" needs to be addressed.

The addition of the ARGO float data study is an improvement and addresses the second part of comment A-27. However, the proposed figures and accompanying text contain an error and also could be made clearer.

For example, the first sentence says "A verification study was carried out by Provincial Airlines Environmental Services Division … ". A verification study of what – surface currents? To verify potential iceberg drift speeds and directions? This should be made clear up front. And note that it is now **Provincial Aerospace Ltd**, not Provincial Airlines. There is still a Provincial Airlines for commercial operations, but since the mid-2000's the part of the company that carries the Environmental Services is Provincial Aerospace. (Note: The term "Provincial Aerospace" was correctly used in the paragraph addressed in comment A-38 … why was it not referred to correctly here?). Also, when using an acronym like DFO, this needs to be spelled out somewhere so that people know it means Department of Fisheries and Oceans.

In the second paragraph, the sentence "The drift speed (Figure 3-X) was observed to be marginally higher overall, …" is unclear. Do you mean the drift speeds determined by PAL were higher than those of Fissel?

Response to Comment A-28: unsatisfactory.
The response to this comment does not correct the problem at all. In fact, the paragraph: "Frequency of presence of sea ice concentrations for the Grand Banks south of 49°N are fairly consistent at approximately 6/10ths coverage. Ice concentrations of greater than 5/10ths are evident by early February and continue through to mid-April, after which they slowly decrease to 2/10ths coverage as per Figure 3-37." is just plain wrong and still confuses frequency with concentration in the second sentence.

The section is called "Concentrations" not "Frequency of Presence". You cannot just change the text from concentrations to frequency because you used the frequency charts … you need to go back and look at the median concentration charts or the ice graphs of ice coverage. The ice graphs referred to in comments A-20 and A-21 show that for the Grand Banks as a whole and for the Hebron study area, seasonal averaged ice coverage is generally less than 10% or less than 1/10 concentration (this is also indicated by the actual median concentration charts in the CIS East Coast ice atlas). The ice graph referred to in comment A-29 indicates that when ice is present in the Hebron study area, sea ice coverage can reach greater than 6/10 during years with large incursions.

Additionally, no attempt has been made to address the last part of comment A-28, which stated: "Also, it is not clear how the information in this paragraph, which purports to describe the seasonal variation in sea ice concentrations over the Grand Banks, relates to Figure 3-37 (whose data is not divided into monthly periods) as indicated in the last sentence. This needs to be amended."

Response to Comment A-31: partly satisfactory.
The revised sentence should spell out "… Canadian Ice Service (CIS) … " and not just use the acronym CIS on its own, unless it was recently described elsewhere.

Also, ice charts from the Canadian Ice Service date back to 1968 at the very earliest. So where do the dates 1964-1987 in the second sentence come from?? This needs to be re-checked and corrected.

Response to Comment A-33: partly satisfactory.
Yes, it is understood that PAL/IIP data were used, that is not the point of this comment. The point is that the data you present only goes up to 2008 when data up to 2009 are available. Why were the data from the last year not included in this study?

Response to Comment A-34: partly satisfactory.
Most of this comment was addressed satisfactorily, however the sentence "This trend for light iceberg distribution …" also needs to be revised since 2010 was a very light iceberg year (as per the figure that is now being included) and so **the light iceberg conditions did not end with the 2008 season as indicated**.

B. Responses to Comments on SECTION 13 of CSR

Response to Comment A-42: unsatisfactory.
Although the requested change from "cyclical" to "variable" was made, a previous comment (Comment A-3) asked that the error in the indicated data span "1983-2008" be corrected everywhere to "1971-2000" so that it is consistent with the cited data source, and (similar to the case of comment A-1 and elsewhere) this was not corrected here in the response to comment A-42.

Response to Comment A-43: unsatisfactory.
The proposed correction to the sentence is incorrect (and I think table 3-50 not table 3-10 is what is being referred to). First, Ice that is less than 30cm thick is not called first year ice, it is called young ice. Also, your own graph (Figure 3-14) shows that mid-March to early May ice thicknesses can reach up to 120cm, not just 70cm. Also, it is noted that the ice thicknesses referred to in graph 3-14 represent the tops of the 30-70cm and 70-120cm thickness ranges for thin first year and medium first year ice respectively. Please rephrase the sentence to read:

"From mid-March through to early May, the bay experiences first year ice which can range in thickness from 30 to 120 cm."

2. Evaluation of Responses to sea ice and iceberg comments NOT made by CIS (Section 3 of Response Document)

B. Responses to Comments on SECTION 13 of CSR

Response to Comment 140 (p.95 of 152) (pdf p.97):
The response to comment 140 is incorrect and does not incorporate the requested changes made by CIS on this issue. The proposed response contains phrases "The duration of the data is 1983 to 2008 inclusive (CIS Ice Charts)." and "These statistics are based on Environment Canada CIS's Sea Ice Charts (1983 to 2008)." *These have not been corrected to 1971-2000 as requested*. **Regarding this issue, CIS commented that** "After reviewing the data in your graphs (Figures 3-13 and 3-14), it appears that you used the Ice Atlas data from 1971-2000 and not the 25-year 1983-2008 chart data that you mention in the text. **This needs to be corrected in the text and in the Sources listed under the Figures.**"